experiments he carried out. Finally he formulates the laws which we use to-day. In the second memoir the formula for the mutual action between two infinitely small elements of conductors carrying currents is proved. Ampère's researches paved the way for much of Faraday's work, and Clerk Maxwell makes full use of his results in his treatise. Clerk Maxwell well called Ampère the Newton of Electricity. The guiding experiments and the theory seemed to start fully equipped from his brain just as Pallas Athene was born fully armed from the head of Zeus.

Small Talk at Wreyland. By Cecil Torr. Second series. Pp. vi + 120. (Cambridge: At the University Press, 1921.) 9*s*. net.

In his second series of "small talk," Mr. Torr, proceeding on the lines followed in his first volume, has brought together a number of pleasantly written discursive jottings on various matters drawn from his own recollections and from the letters and diaries of his father and grandparents. An antiquarian and a scholar, he writes with a light and pleasant touch on such matters as local lore and history, as well as of events in the larger world. The value of these notes lies in the light they throw on the social habits and customs of the middle of the last century; they deal with those illuminating details which are apt to evade the more formal historian. Interspersed are observations of and reflections on happenings which have befallen Mr. Torr during his travels in the Mediterranean and in Palestine. All topics, whether of a serious or a lighter character, are touched upon in a manner which can only be described as urbane. On one subject alone does Mr. Torr's urbanity desert him, and that is when he is moved to comment upon the Government regulations for the cultivation of the land during the war.

Chemical Reactions and their Equations: A Guide and Reference Book for Students of Chemistry. By Prof. I. W. D. Hackh. Pp. viii + 138. (Philadelphia: P. Blakiston's Son and Co., 1921.) 1.75 dollars.

"The inability to balance a chemical equation is a most common difficulty to students of chemistry." The author has attempted to remedy this very common weakness, and in addition to a concise explanation of chemical notation, including difficult cases of oxidation and reduction and ionic reactions, has provided a list of more than four hundred classified and indexed chemical equations. The book should prove a useful companion to degree students. In the list of solubilities "to be memorised" one finds: "BORATES are SOLUBLE," which is not strictly correct, since most borates are insoluble.

The Practical Chemistry of Coal and its Products. By A. E. Findley and R. Wigginton. Pp. 144. (London: Benn Bros., Ltd., 1921.) 12*s*. 6*d*.

The analysis of coal, coke, ammonia liquor and ammonium sulphate, tar and its distillation products, gas (including calorimetry), pyrometry, and water analysis, are the topics dealt with in this book. The volume is very attractively printed and illustrated and should prove most useful in works laboratories.

Letters to the Editor.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Definition, Resolving Power, and Accuracy.

In scientific writings the term "definition" most often refers to the clearness with which details are shown by optical instruments; but by a convenient generalisation it might be taken to mean the ratio of the greatest to the least quantity which any kind of apparatus can render apparent at the same time, being thus distinguished from "sensitiveness" or "resolving power," which is determined simply by the smallest quantity measurable without reference to the size of the field.

In this sense the question of "definition" enters into every kind of measurement. In telescopes and microscopes, for example, it would denote the angular or linear size of the field of view compared with the smallest corresponding quantity which can be clearly distinguished; or, in a balance, the greatest arc through which it can swing compared to the least angle of swing giving a trustworthy measure of change of weight.

Since all measurements have in the end to be recorded by the senses either of sight, hearing, or touch (smell and taste have not yet been examined quantitatively), it is of interest to inquire what kind of definition can be expected in their case, and the following notes contain some of the results of various observations on the subject made at intervals during many years.

Sight and hearing are both dependent on wave-motion, and the sensations produced vary with the intensity, frequency, direction, and duration of the waves. The total range of sensible intensities is enormous; for it is seldom that a night is so dark, or a silence so complete, that absolutely nothing can be seen or heard, yet the eye can work without injury in bright sunlight, and the ear can hear with such noises as thunder or great explosions. In these cases the ratio of the greatest to the least appreciable intensity must be of the order of millions.

Although, however, the perceptions of intensities have such wide limits, the differences which can be recognised at any one time or in any constant conditions are much more limited.

In many respects the senses may be compared with a musical instrument which, while of restricted compass, can be tuned to almost any absolute pitch, so that though for any one tuning comparatively few notes can be sounded, yet by adjustment these notes may take any desired position in the audible scale. Each sense, in fact, seems to adjust itself to some kind of level suitable to its surroundings, and to be able (so far as my own observations go) to discern differences of from 5 to ⅓ per cent. of the range then appreciable.

The same order of definition was found not only for each sense but also for the co-ordination of the senses with muscular action.

The following experiment on the greatest difference between the intensities of light which can be perceived at the same time always gave fairly consistent results. A long tube AB, Fig. 1, about two inches in diameter, and well blackened inside, was provided with a white paper flange at A, and a movable piston, C, also covered with white paper. A disc of white paper, D, of rather less diameter than the tube was placed at a short distance from the flange, and could be illuminated by sunlight or other means. A small hole in the disc allowed the eye to look in the direction of the axis of the tube, and when the piston was flush with the flange the view obtained was of a uniformly white surface lit by the light scattered from D.

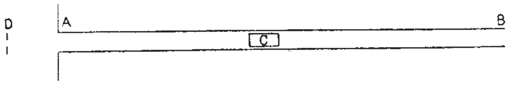


FIG. 1.

The experiment consisted in withdrawing the piston until it became invisible, the relative intensities of the illumination of the flange or piston face then being DC^2/DA^2. For sunlight the ratio was about 220 to 230 and for candle-light about 170, thus indicating a definition 0·45 and 0·5 in each case.

Analogous experiments were tried on sound by the use of two lever clocks placed at some distance apart. The distance was then noted at which the ear had to be placed from each in order that one set of " ticks " might be drowned by the other. Musical sounds of the same pitch and loudness were tried in the same way. It is always difficult to make indoor experiments on sound on account of the echoes from walls and furniture, but on the whole it appears that the definition for the intensity of sound was not so good, though of the same order, as for light.

As regards the definition for wave-length, that is, the recognition of colour and pitch, the range for visible light waves is much narrower than that for sound. The lengths of all the visible rays lie within the ratio of two to one, while in sound a compass of more than ten octaves is audible. Few people when viewing the whole spectrum at once will distinguish more than seven or perhaps eight colours, but when only a small portion covers the visible field, the variations of tint are much more marked. I have been told by one well-known observer that he could distinguish between the yellows a very short distance on either side of the D lines. This would correspond to a difference of wave-length of five or six parts in a thousand, but since wave-lengths at the orange and green end of the yellow do not differ by more than a hundred parts in a thousand, the definition implied is not better than five per cent.

Few ears are so unmusical as not to be able to distinguish intervals of a semitone (wave-length ratio about ten per cent.), but fewer still can at once distinguish between a major and minor semitone (about one per cent.), and even fewer between the true semitones and their equal temperament substitute.

The sensibility of the ear to change of pitch varies largely in different parts of the audible range, and is at its best in the two octaves above and the octave below the middle C.

In both light and sound the judgment is improved if there is a fixed standard for reference. No standard would be required by the normal eye to determine whether an object was red, yellow, or blue, but doubt might easily be felt as to the exact shade of the colour if the objects were seen separately at considerable intervals of time, and the longer the interval the less as a rule is the judgment to be trusted.

The relation of musical notes sounded in succession in the same way is less correctly estimated when the time intervals between them are long than when they are short, except for those who have the sense of absolute pitch. I have no good data as to the range over which this rather rare sense extends, but have reason to believe that in some cases and in the vocal compass it is accurate to within one per cent.

The same sort of memory which enables a definite colour or pitch to be recognised without any external standard of comparison is requisite also for the mental division of intervals of space or time. I have known people who could mark off inches and count seconds by memory with errors of not much more than one per cent., but they could not deal with feet or minutes with anything like the same accuracy.

In estimating fractions of a second a good transit observer will approach a two per cent. standard, but whether the second is the best time interval for mental division may be questioned.

As regards the division of space by estimation I may mention the following simple experiment which I have often repeated at intervals of years and always with the same result.

On a sheet of ruled foolscap draw two straight lines intersecting near one end of the sheet, and about six inches apart at the other (Fig. 2). Mark the intersections of these lines with the ruling by well-defined circular black spots, and from another sheet cut out a slot about six and a half inches long and half an inch wide. This when superposed on the first will allow one pair of spots to be visible at the same time. Now, always keeping the eye at the same distance from the paper, mark in succession the point estimated to be midway between each pair. These marks should all lie on the bisector of the angle between the first drawn divergent lines.

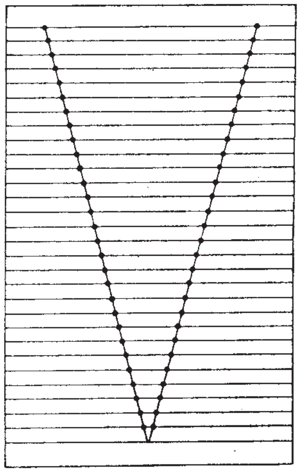


FIG. 2.

In all my trials I found that so long as the subtense of the pair of spots was small enough to allow of both being seen clearly at the same time, the angular error in the estimation of the bisection was more or less constant and ranged from 1/500 to 1/1000 (*i.e.* from $\frac{1}{50}$ to $\frac{1}{100}$ inch at ten inches, the distance of the eye from the paper). The error, however, increased rapidly when the spots were so far apart that they had to be viewed in succession. This occurred when their subtense was about 20°, and the best bisections were made with a subtense of 15° to 12°.

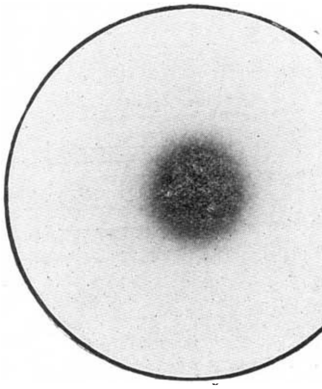
For smaller angles the definition was not so good, for though the linear error on the paper did not change much, it had to be compared with the smaller distance between the spots. In my own case, for angles greater than 20° the error in the bisection was always to the left, but this probably is a personal matter.

As for space, so for time, there must be some particular interval which is best suited for division of estimation, and my impression is that this is somewhat, but not much, longer than a second. As an example of the importance of the correct estimation of the bisection of a space, I may refer to the results of the recent eclipse expedition which were supposed to confirm Einstein's theory regarding the effect of gravity on light.

According to the account given in the Report (Phil. Trans. A, vol. 220) only one of the sets of eclipse photographs was quite satisfactory, and this was taken by a 4-inch object glass having a focal length of 19 ft. At this distance one-thousandth of an inch would subtend one second of arc nearly, and a 4-inch lens would be just sufficient to separate objects this distance apart. Thus if the lens and photographic plate were perfect, the image of a star would be represented by a circular spot 0·001 in diameter surrounded by one or perhaps two faint rings. In the actual negatives, from irradiation and other causes, the star images were easily visible to the naked eye, and were (speaking from inspection only) about a hundredth of an inch across.

The object of the photograph was to determine the position of the stars to within a small fraction of 1″, so that 10 per cent. in the estimate of the bisection of the 0·01 inch image would represent an angular error several times as great as the whole deviation of the ray suggested by the theory.

The image of a star on a gelatine plate is not a sharply defined disc, but a group of dots crowded together towards the centre but more sparsely scattered round the circumference. This is illustrated by Fig. 3, which is the enlargement (×300) of the direct contact print on a slow "Imperial" plate of a hole 0·0015 in diameter pierced through thin copper foil, and is therefore probably rather more sharply defined than an image formed by a lens.



Thus the evidence for Einstein's theory (so far as the eclipse results are concerned) turns on the question of how much closer than ten per cent. can the bisection of such an area as the figure shows be estimated by the eye.

What magnification was used in the measuring apparatus is not stated in the Report, but assuming that it was between 20 and 40, Fig. 3 would have to be held between 25 and 50 inches from the eye in order that it should appear of the same size as the star image would in the measuring microscope.

Another matter of some interest is the acuteness with which the senses perceive vertical or horizontal acceleration, or in other words the variation of the intensity and direction of the forces acting on the body.

In the inquiry into the vibrations caused by trains in the London Tube Railways, it was found that residents in the neighbourhood began to complain when the vertical movements at the rate of 15 per second amounted to so little as one-thousandth of an inch (amplitude 0·0005), corresponding to a maximum acceleration of $g/12$, but that much smaller accelerations could be felt. Up to a frequency of 40 per sec. or thereabouts, the quicker the vibration the smaller was the amplitude which was perceptible. With frequencies sufficient to give rise to an audible note, the character of the sensation gradually changed.

Church towers rock when the bells are rung, and on one tower rather celebrated in this respect I measured the maximum horizontal amplitude of $\frac{1}{12}$ of an inch, the frequency being rather less than 3 per second. The maximum acceleration in this case also was about $g/12$ and produced feelings of sea-sickness in many people. Accelerations of less than a tenth of this amount, however, were quite noticeable.

I have no notes on the effects of vibrations of long period, but I should expect that any periodic motion which involved an acceleration of $g/200$ would not pass unnoticed, if the conditions were favourable and the attention directed to the subject.

The following are a few examples of the accuracy with which the senses can direct muscular action. In match-rifle shooting sequences of 100 bulls'-eyes (24 inches) at 1000 yards are not unknown. In this case the symmetry of the sight and the target are an assistance in aiming, and since part at any rate of the deviations of the shots are due to wind, etc., the actual alignment of the sights cannot be much greater than 1 in 4000.

By the assistance of a champion billiard player I found that, in playing at a ball under the cushion from balk, he seldom missed the centre aimed at by more than $\frac{1}{8}$ inch. This is equivalent to an accuracy of 1 in 1000. The trials were made by using a paper disc, backed with carbon-tissue and a hard wood support, as the target, the disc being of the same diameter as a billiard ball. The point of impact was shown by the black dot left by the carbon on the reverse side.

A good bowler can generally hit a single stump at 22 yards. A cricket ball has, I believe, a diameter of $2\frac{5}{8}$ inches, so that taking the thickness of the stump into account the accuracy in this case is about 1 in 380.

As regards archery, the best of modern archers[1] will scarcely keep his arrows within a 3-ft. circle at 100 yards. If he did the accuracy would be 1 in 200.

For a game-shot who can bring down 75 per cent. of his birds at 40 yards, the implied accuracy of aim is about 1 in 70.

In all these cases the attention is concentrated on a field of only a few degrees, and the effect of the restriction of the field on the accuracy of the estimation of the position of objects within it is worthy of more investigation than it has yet received.

In judging the qualities of the instrument with which measures are made, a distinction should be drawn between accuracy or resolving power and definition. The accuracy with which a weight can be determined by a balance, or a resistance by a Wheatstone bridge, is greater than one part in a million, but the definition is, for the balance, the length of the arc in which it can swing compared to the least angular motion which gives a trustworthy indication, and, for the bridge, the length of the resistance wire on which contact is made compared with the least variation of the position of contact which can be shown with certainty on the galvanometer. Accuracy will depend, among other things, on the accuracy of the weights and balancing resistances employed, while the definition is decided by the workmanship and design of the instrument alone.

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[1] How old is the legend of the pole, string, and bird, as a test of an archer's skill? Tom Sawyer, it may be remembered, improved on this, saying that Robin Hood "would take his yew bow and plug a ten cent piece every time—mile and a half." In Joshua xx. 16 there is a reference to seven hundred left-handed men of Benjamin. "Every man could sling stones at a hair's-breadth and not miss." This, I think, is the only Biblical reference to accuracy of marksmanship.

In optical instruments, resolving power is judged by the smallest angular or linear quantities which can be distinguished by their means, and definition by the relation which these quantities have to the size of the field over which they are distinguishable. If a telescope, for instance, can distinguish seconds of arc, its resolving power is 206,000 nearly, but if the field over which this resolution extends is only half a degree, the definition would be 1800 (*i.e.* $206{,}000/57 \times 2$).

Although the performance of any combination of real lenses depends on design and workmanship, it is not difficult to find the limits beyond which even perfection in both does nothing to increase resolving power.

The function of a perfect lens is to change the radius of curvature of a spherical wave surface. Let D (Fig. 4) be the diameter of the lens where the change is effected, f the new radius of curvature, and o the geometrical focus. From every part of the wave surface at D partial waves may be supposed to spread, all of which will reach o in the same phase. Confining the attention for the moment to those rays which start from the opposite ends of a diameter of the lens, the partial waves from either end will be in opposite phases at a distance a, in the focal plane, from o if $2a \sin \alpha/2 = \lambda/2$, where α is the angle subtended at o by D, and λ the wave-length of the light. Also, since $\sin \alpha/2 = D/2f$, $a/f = \lambda/2D$ and $a = f\lambda/2D$. Thus $\lambda/2D$ and $f\lambda/2D$ are the least angular and linear distances from the geometrical focus at which a total absence of light can be found.

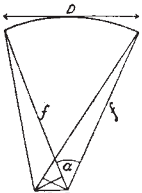

FIG. 4.

If the partial waves are received from the whole marginal annulus of the lens, in place of those from the extremities of a diameter, the value of a_1 is slightly increased, and the image about o of a distant point of light is a bright circular area surrounded by a series of rings. (The rings are caused by recurring coincidence of the phases of the partial waves at certain distances from o greater than a.) The bright centre and rings are identical with those seen in the well-known experiment in which a bright point appears in the centre of the shadow of a disc, illuminated by a bright distant source.

If the whole area of the lens is employed the diameter of the bright centre is further increased, but the intensity of the rings is much reduced. This case, as it applies to telescopes, has been considered by Airey in a paper on "The Intensity of Light in the Neighbourhood of a Caustic," published about the middle of the last century.

In whatever manner, however, the lens is used, whether with a central stop, or with its whole area uncovered, a/f and a are the least angles and distances which separate the geometrical focus from the truly dark boundary of the image, although, owing to the rather rapid diminution of intensity from the centre to the circumference of the image, it is possible to recognise angles smaller than $\lambda/2D$ (this applies to telescopes in which α is always small), or distances less than $\lambda/2$ by the use of microscopic objectives of large angular aperture, where $\sin \alpha/2$ approaches unity.

The appearances presented at the focus when two objects in such close proximity are examined can scarcely be described as the images of the objects, but rather as interference phenomena which require interpretation.

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Discoveries in Tropical Medicine.

IN NATURE of April 29, p. 549, Sir E. Ray Lankester criticises an obituary notice on Sir Patrick Manson which appeared in the *Times* of April 10. The statement chiefly objected to is that "modern tropical medicine was born the day that Manson discovered the part played by the mosquito in the transmission of *Filaria sanguinis hominis*." But all parasitologists know that it was Manson who, forty-four years ago, proved by experiment the part played by mosquitoes in the propagation of filariasis. This discovery is not only the pride of tropical medicine, but the very breath of modern medicine and one of the most glorious achievements of British science.

Strange to say, Sir Ray Lankester to-day repeats, almost word for word, a mistake he published twenty years ago in the *Times*. He says: "The fact is that Manson's 'suggestion' that the Filaria of elephantiasis [*sic*] is actually carried by mosquitoes from the blood of one person to that of another remains to this day a 'suggestion.' It has not been established as a fact."

It is surprising that a naturalist of repute, who writes frequently on matters medical, should make this mistake. Surely Sir Ray Lankester must have come across books on parasitology written within the last twenty or thirty years. How can he say that the agency of the mosquito, in the dissemination of filariasis, has not yet been established? Was it not surmised by the Chinese ages ago? Was it not suggested by Bancroft of Brisbane (Queensland) in 1877? Was it not independently and experimentally proved by Manson, in China, that very same year? Only one thing remained uncertain for some years, and that was the actual method by means of which the young filariæ, after reaching a certain stage of development within the body of their insect host, left the mosquito to invade man; but this was fully established and admirably demonstrated between 1899 and 1900 by Manson, Low, Bancroft, Annet, and Dutton, James, Noè, Grassi, myself, and others. Dr. Low's beautiful celloidin sections of infected mosquitoes showing the worms, either quiescent between the massive thoracic muscles of the insect, or actively migrating to the latter's mouth parts and fixed while passing beneath the cephalic ganglia, gliding down within the labium, or escaping through a rent between the labella, at the distal extremity of the labium, have been exhibited repeatedly, not only at the Royal Society and elsewhere in this country, but also, on several occasions, in France, Italy, and Belgium. Moreover, they have been photographed and even reproduced in colour by skilled artists, and both photographs and drawings have appeared in books on tropical medicine and parasitology published since 1900.

Sir Ray Lankester refers to two other vexing questions, namely, the discovery of the part played by mosquitoes in the propagation of the intermittent fevers and that of tsetse flies in the transmission of certain trypanosome diseases of animals and man such as nagana and sleeping sickness. With regard